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AUTHOR Prideaux, Gary D.
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ABSTRACT

This paper argues that we are moving toward a more experimental approach to language, one which is characterized less by introspective judgments and more by empirical evidence. The first section of the paper is a general discussion of the directions linguistic theories are taking. The second section discusses some of the fundamental problems involved in linguistic theory. The third section is a sketch of a linguistic model based on the assumption that the function of language is to communicate information. The fourth section discusses some syntactic rules represented in the model. The fifth section discusses the testing of the psychological validity of the rules. The sixth section summarizes the focus of this paper and concludes that empirical evidence is of central importance in testing linguistic theories. (TS)

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LINGUISTIC THEORIES: WHERE ARE WE GOING?

Gary D. Prideaux
Department of Linguistics
University of Alberta
Edmonton, Alberta, Canada

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It is important to know what others are doing,
but it is more important to know what Nature
is doing.

--Philip J. Greenberg

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LINGUISTIC THEORIES: WHERE ARE WE GOING?

1. Introduction. Although it is relatively easy to state, with a bit of myopia, just where linguistic theories are, it is somewhat more difficult to say why we are where we are, and even more difficult to predict--or better, to guess--where we are going. For this reason, it is useful to summarize my position before developing it in detail.

Linguistics has passed through a long and complex period of theorizing based, in the transformational framework, on formal syntactic analysis--analysis of the language product. Coupled with formal analysis have been claims about the psychological reality of rules and structures embodied in formal descriptions. This Zeitgeist has been so pervasive that linguists have not felt uncomfortable about asserting that their grammatical rules are "psychologically real" in the sense that the rules reflect either actual psychological processes or something called the "linguistic competence" of the native speaker, or both. At the present time, this shell of exuberant confidence in the psychological reality of formally motivated rules is crumbling. More and more scholars are asking for evidence in support of such claims. And such evidence is sadly lacking.

While it is difficult to guess exactly where we are going, some general signs are fairly clear. I feel that we are moving toward an empirical investigation of language,

with the persuasive power of formal arguments giving way to the strength of empirical results. In other words, I think, feel, and hope that we are moving toward a more experimental approach to language--an approach characterized not so much by formal niceties and introspective judgments as by strong empirical evidence. New linguistic theories are being constructed which feel no obligation to such heretofore enshrined concepts as deep structure and transformations. Such theories will be built piece by piece in a tedious way, with trial and error, minor victories, stinging defeats, and eventual progress. This attitude will, I suggest, replace the plethora of "heroic solutions" that linguists have offered over the past twenty or so years. We are, in short, moving into a linguistics which links theory construction with empirical facts to the extent that the theory is psychologically interpretable and testable.

In order to make my case for this direction in linguistics, I wish first to characterize some of the epistomological dilemmas in which current transformational theory finds itself, and then propose the outline of a psycholinguistically plausible alternative theory. Finally, I will provide some examples of internal and external evaluation for certain rules of the proposed theory. In all that follows, however, a basic assumption must be kept in mind: I consider linguistics to be more than an exercise in formal description. To be called a science, linguistics must be responsible to and constrained by empirical facts, such as those gained by

the experimental investigation of language phenomena.

2. Fundamental Problems in Linguistic Theory. Over the past several years, much research in the fields of reading and other disciplines concerned with language use has been heavily influenced by that large and heterogeneous body of linguistic theorizing called transformational generative grammar. Transformational grammar has often influenced the theoretical approaches taken to various experimental problems and has even governed the choice of specific research topics. Furthermore, such experimental work is frequently interpreted in terms of the theoretical pronouncements of linguistics. The role played by transformational theory in experimental studies has been enormous.

One reason for the great influence of transformational grammar on experimental studies is that the linguist has developed extremely powerful descriptive tools for the formal analysis of language. A theoretical apparatus has been constructed for formally representing relations both within and among sentence types. Included in the theoretical arsenal are such concepts as deep structure, transformations, and rule ordering. The resulting formal system, however, has the computational power of an unrestricted rewriting system--a system generally acknowledged to be entirely too powerful for the description of human language (since an infinity of non-human language structures can also be characterized by the system). But the linguist's tools and system are addressed to the description of the language product--mostly

sentences. While he is interested in describing the structure of sentences, the linguist has not shown a corresponding interest in the psychological processes involved in language use. These latter are generally swept under the rug as matters of "mere performance" rather than of the more highly valued "competence." Yet since his descriptive system is so powerful, the linguist feels compelled to impose constraints. He requires, for example, that his grammar relate in a systematic way those sentences which are thought to be syntactic paraphrases of each other. He insists that his grammar account for structural ambiguities in a way that provides some syntactic differentiation for sentences which have multiple meanings. In general, the concepts of deep structure, transformations, and rule ordering are used to accomplish such ends.

For example, in analyzing English, the linguist notices that two sentences like:

1 a. It is obvious that Mary left early.

b. That Mary left early is obvious.

are paraphrases, sharing roughly the same meaning. In order to relate the two surface structures, a common abstract deep structure is proposed, roughly of the form:

1 c. [[it [Mary left early]] [is obvious]]
S NP S VP

In order to obtain the two surface structures, a transformation of EXTRAPOSITION is posited which moves the embedded S

(along with its transformationally introduced complementizer that) to the end of the main clause. Such a rule is (usually) regarded as optional and is considered "meaning-preserving." If we extrapose the embedded S, sentence 1b results, but if we do not move the clause, we can obtain 1a only by using another transformation, that of IT DELETION. This example illustrates the use of deep structure, transformations, and rule ordering in the analysis of the language product.

The treatment of ambiguity also utilizes the same tools of deep structure and ordered transformations. We will return to questions of syntactic ambiguity below.

When he is able to characterize the syntax of paraphrase and ambiguity by a set of syntactic rules, the linguist often claims that he has also characterized the "linguistic intuition" or "linguistic competence" of native speakers. But not only is the status of such a claim extremely unclear, as many writers have pointed out (cf. Derwing, 1973; Prideaux, 1971; Watt, 1970), but more importantly, the linguist has not included anything in his formal description about language processes or states obtaining within the language user. All he has done is to analyze and relate the structures of sentences. His descriptive tools are expressly designed to deal with formal, not psychological, problems. Furthermore, since attention is largely addressed to formal structures, little attention is paid to the functions which language structures serve. That language has as its basis a communicative function, and that various

syntactic structures serve to signal different kinds of functional information may be obvious to the man in the street, but the linguist generally does not concern himself with such functions any more than he does with psychological processes. His interest is in structure. While one might be so foolish as to claim that the description of a chocolate cake is equivalent to a statement of how the cake was made or tastes, the proof is unfortunately not in the eating.

Linguists do not seem terribly interested in experimental results, even if their theories have spawned the experiments. In fact, some linguists have gone so far as to claim that linguistic theory is so self-contained that experimental evidence is totally irrelevant. Such a position involves the assumption that when faced with negative experimental evidence regarding some theoretical issue, it is always the experiment and not the theory which is out of step. For example, Fodor and Garrett stated

> A grammar is simply an axiomatic representation of an infinite set of structural descriptions, and the internal evidence in favour of the structural descriptions modern grammars generate is so strong that it is difficult to imagine their succumbing to any purely experimental disconfirmation. Rather, one would best interpret negative data as showing that an acceptable theory of the relation between competence and performance models will have to represent that relation as abstract, the degree of abstractness being proportional to the failure of formal features of derivations to correspond to performance variables (1966, 152).

The claim here is simply that grammars are so well-motivated internally that no experimental evidence against some

theoretical claims can ever be valid, although presumably evidence in favor of a theoretical pronouncement would be gladly accepted. One reason, then, why linguistic theory has been so little influenced by experimental work is that the theory has been declared immune to disconfirmatory experimental evidence. Over the past few years, however, this attitude has begun to change, and one reason, although not necessarily a "good" one, for the change is that linguistic theories have proliferated into a multitude of competing and contentious camps, each making different claims about the nature of syntactic and semantic representation, transformations, and the like. Consequently, confidence in the "internal evidence in favour of structural descriptions" has begun to erode. In fact, internal evidence is extremely weak for <u>any</u> particular theoretical stance nowadays, and by necessity many linguists are now claiming that external evidence is crucial in evaluating theoretical claims. Unfortunately, the very nature of linguistic theorizing is such that it is not at all clear just how such external evidence can be related to formal theories. How, for example, is one supposed to give any sort of psychological interpretation to an abstract level of deep syntactic structure, since such a structure is never available for investigation? Or how is one to interpret a notion of transformation which allows constituents to be moved about in the course of a derivation? Or how, for that matter, is one to interpret a syntactic derivation psychologically? The notions of deep

structure, transformation, and derivation obviously play a role in a formal description of the language product, but they make no sense at all in terms of the language process.

One early attempt to interpret formal grammars, the so-called "Derivational Theory of Complexity," involved the supposition that the degree of syntactic complexity as represented in a formal derivation might correspond to psychological complexity in sentence production or comprehension. Under such an interpretation, a sentence such as John saw the dog should be easier to produce or to comprehend than the passive The dog was seen by John since the passive form involves a transformation (PASSIVE) not used in the active. However, the passive sentence is longer, less frequent in the language, and involves a complex of co-occurring structures (be+en, by, and the inversion of two NPs). Furthermore, according to the Derivational Theory of Complexity, a truncated passive such as The dog was seen should be even more difficult to process than the full passive since in the truncated version a deletion transformation has operated. Experimental evidence revealed that little credence could be given to the Derivational Theory of Complexity, and in fact much evidence has refuted the theory (c.f., Watt, 1970; Fillenbaum, 1970; Reid, in progress). If the Derivational Theory of Complexity is not an appropriate interpretation of a set of rules, what is? How could one in principle evaluate psychologically a notion like deep structure or ordered transformational rules? Could it be

that linguists, and those they so glibly advise, have the theoretical cart before the empirical horse? Perhaps a theory should be constructed which disallows in principle untestable notions like deep structure, syntactic derivation, and transformation. By so doing, one would automatically rule out a priori a great number of potential interpretations. A Derivational Theory of Complexity, for example, would not be a viable interpretation of such a theory.

Most sciences rely heavily on theoretically and experimental work going on hand in hand. Most scientific theories are empirically testable in at least some of their statements, even though no modern scientist would require that every statement in his theory be empirically testable. But on the other hand, no astrophysicist, for example, would ever expect his theory of stellar interiors to be in principle empirically immune. No cosmologist would ever seriously propose a theory that had no observational consequences. Relativity theory is supported by specific observational tests. And Hubble discovered the expansion of the universe observationally, with the theoretical explanation residing in Einstein's field equations. In such sciences, theory and experiment feed, bolster, and even confound each other. But they do interact. In linguistics, on the other hand, the notion of what constitutes an empirical observation has generally been relegated to statements about the form of sentences and intuitively felt relations among them.

In summary, one of the reasons why experimental

evidence has had little impact on linguistic theory is that the linguist is primarily interested in the language product, and his descriptions are of sentences. Most empirical investigations, however, are directed toward such facets of language use as comprehension, processing difficulty, and language acquisition. Such studies focus attention on the language user rather than on the particulars of the language product. While the modern linguist may make extravagant psychological claims for his theories, such theories are usually treated as invulnerable to psychological evidence. They cannot be interpreted and therefore they cannot be refuted.

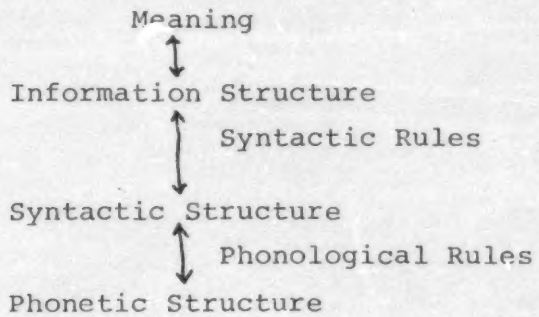
Given the current unrest around the theoretical campfires today, plus the increasing sophistication of experimentalists dealing with language behaviour, more and more interest is being directed toward the experimental study of language. At the same time, some linguists are allowing their minds to be opened by the studies carried on outside their own camps. The time is ripe for linguistics to gain its maturity by acknowledging the input from experimental studies.

3. Information Structure. Given these preliminary remarks, I now turn to the sketch of a linguistic model which attempts to deal with the problems outlined above. The model was, and is being, developed in our psycholinguistics research group at the University of Alberta. Our basic assumption is that the function of language is to communicate information. A natural language, in the model, is assumed to consist of two levels of organization: the level of

(surface) syntactic representation and the level of information organization.

Before outlining the model, it is important to make explicit the constraints imposed on it. First, the model must be psychologically interpretable. For this reason, the syntactic generalizations we propose are just those which are extractable at the surface and thus potentially learnable. Second, to avoid any possible Derivational Theory of Complexity interpretation, the notions of transformation and deep syntactic structures are avoided. Third, since there is no evidence of language users "moving" constituents about as they produce or perceive sentences, all constituent movement rules are disallowed. Fourth, while syntactic representations must be dealt with, there is more to a human language than syntax. In particular, various syntactic configurations exist to signal distinct kinds of information. For example, a certain word order in English signals a yes-no question while a different word order represents a statement. The model, then, is responsible not only to the syntax of the language but to the functions that syntactic structures convey.

The model can be represented roughly as follows:



The grammar of a language is a specification of surface syntactic generalizations which are paired with specific kinds of information. We take meaning not as a static notion but rather a dynamic one since the meaning of an utterance in distinct contexts may indeed be very different.

The information structure level consists of four nested parts: denotational information, relational information, sentential information, and contextual information. Denotational information refers to the information conveyed by specific lexical items. For example, a NP like the boy consists informationally of a lexical head N (boy) plus the denotational information "singular, male, human, definite," etc.

Relational information refers to the various grammatical functions played by syntactic constituents. Such relations include subject, direct object, indirect object, predicate, etc. The function of a particular constituent is paired by syntactic rules to its position in surface syntax.

Sentential information refers basically to sentence type: declarative, interrogative, or imperative. Again, syntactic rules linearize constituents in terms of sentential information. If the sentential information for a particular sentence is SI = Q (the sentence is a question), then certain syntactic rules reflect this choice in the surface word orders.

Contextual information refers to the contextual or discourse factors which govern the syntactic shape of a sentence. For example, while the two sentences:

2 a. What Jason stole was the fleece.

b. It was Jason who stole the fleece.

both share the same relational information, namely that Jason is the subject and the fleece is the direct object of steal, the former is an appropriate answer to:

2 c. What did Jason steal?

while the latter is not. The two sentences differ in their contextual information to the extent that they differ in focus.

Given these basic notions, the next important question is: How is information structure paired with surface structure? or, What do the syntactic rules look like? It is to this question that I now turn.

4. Some Syntactic Rules. A syntactic rule in our model is a statement which pairs information structure with surface constituent orderings. The IS level is not represented as a labelled tree, but a surface structure--a syntactic representation--is representable as a labelled tree or alternatively as a labelled bracketing. Syntactic rules must represent generalizations true at the surface. They may not move constituents about, derive one form of a sentence from another, nor depend on an abstract level of syntactic representation. Surface structure is the only level of syntactic representation in the model.

Syntactic rules are of two basic sorts: linearization rules, which pair some part of information structure with

some part of syntactic structure, and redundancy rules, which state surface redundancies independent of the information structure. To elucidate certain of these rules, let us consider the following data base:

3 a.	Declaratives	
	i. Jason has stolen the fleece.	NP Va V NP
	ii. Susan left.	NP V
b.	Yes-No Questions	
	i. Has Jason stolen the fleece?	Va NP V NP
	ii. Did Susan leave?	Va NP V
c.	WH Questions	
	i. Who stole the fleece?	WH V NP
	ii. What did Jason steal?	WH Va NP V

Beside each example is a syntactic representation of the sentence (where NP is a noun phrase, WH is a questioned NP, V a main verb, and Va an auxiliary verb). If we now assume the notions of subject and (direct) object as basic relations (relational information), we can state several surface syntactic rules.

Notice that in all the sentences, a subject NP is to the left of the main verb. This is true for sentences which have an overt subject, and consequently not true of imperatives. A rule for representing this fact is:

R 1. SUBJ $\longleftrightarrow$ $[_{S}$ X NP VP

The rule states that given a subject (SUBJ) NP, it is placed to the immediate left of the VP, where VP (verb phrase) is

taken literally as a main verb and its adjacent auxiliaries (if any). The VP does not contain object phrases. Notice further that the rule is bidirectional: it can be read from right to left (from syntactic structure to information structure) or left to right (from information structure to surface constituent ordering). X is a (possibly null) variable.

Furthermore, if a particular information structure contains a direct object which is not a questioned (WH) NP, then the direct object is ordered to the right of the main verb. This generalization can be represented as:

R 2. DO $\neq$ WH $\longleftrightarrow$ $[_{S}$ X VP X NP

The variable X between the VP and the direct object can be empty or it can be another constituent, such as an indirect object.

Turning now to questions, where SI = Q, we notice that English (and all other languages that I am familiar with) exhibits two kinds of questions: yes-no questions and wh-questions. Yes-no questions seek information regarding an entire proposition, while wh-questions seek specification of a particular constituent (the wh constituent). In both kinds of questions the subject NP still precedes the main verb (as specified by R 1) and the direct object (if not a wh-form) follows the verb. The word orders signalling a question often, but not always, involve the placement of an auxiliary verb (Va) before the subject NP. In a wh- question the wh-

element is the left-most NP in the sentence. These generalizations can be represented as two linearization rules:

R 3. NP = WH $\longleftrightarrow$ [$_S$ X WH

where: X does not contain a NP.

R 4. SI = Q and SUBJ $\neq$ WH $\longleftrightarrow$ [$_S$ (WH) Va NP

R 3 simply states that a wh-form is the left-most NP in a sentence. Such a rule is probably a special case of a more general rule which states: Place the focused NP to the left. R 3 also applies in relative clauses, where the wh-form is left-most in the relative clause. R 4 states that for questions in which the subject is not a wh-form (i.e., in all yes-no questions and in all wh-questions where the subject is not the questioned form), as auxiliary precedes the subject NP. Such rules automatically account for sentences like 3ci. In this case the wh-form (who) is the subject, so rules R 1, R 2, and R 3 apply, but R 4 does not. The rules as formulated indicate that we do not get an auxiliary verb before the subject NP in wh-questions where the wh-form is subject.

Other rather obvious surface generalizations can be stated as well. For instance, it is the left-most verb, be it main verb or auxiliary, which receives the tense suffix in an English sentence. This generalization can be stated as:

R 5. T $\in$ Dp $\longleftrightarrow$ [$_S$ X [v + T]

where: X does not contain a verb.

This rule can be read as: if tense (T) belongs to the denotational information of the predicate (Dp), then the tense morpheme is suffixed to the left-most verb (v), such that the phonological shape of the verb and the tense are bracketed as the syntactic category V.

Auxiliary verbs are treated in the information structure model as part of the denotational information of the predicate. If the progressive aspect is selected, this information is lexicalized with the auxiliary verb be and a purely syntactic redundancy rule attaches the progressive suffix ing to the next verb to the right. Likewise, the perfect aspect is lexicalized as have, with the en suffix attached to the next verb. These lexical items, plus the modals and the auxiliary do, are part of the lexicon. Redundancy rules account for the proper order of auxiliaries and for the correct aspectual suffixes. Such rules are:

RR 1. [X (M) X (have) X (be) X V

RR 2. $\emptyset \rightarrow \begin{bmatrix} en \\ ing \end{bmatrix} \quad / \quad \begin{bmatrix} have \\ be \end{bmatrix} \quad X \quad [_{V} ___]$

Rule RR 1 provides the correct linear order of auxiliaries while RR 2 attaches the correct suffix to the first verb following an aspectual verb. Finally, if several auxiliary verbs immediately precede the main verb with no intervening NPs, they are bracketed with the main verb as a VP, as represented in:

RR 3. $[_{VP}$ (Va) (Va) (Va) X V X]

The rules sketched here, unlike those in transformational theory, pay equal attention to the form of sentences (syntax) and to the function served by various syntactic configurations (information structure). Syntactic rules are motivated to the extent that they signal various kinds of information--syntax does not exist in a vacuum. The rules state generalizations which are true at the surface and as such they are plausible candidates for psychological reality. They do not depend on either an abstract underlying level of syntax or on transformational derivations. As surface syntactic statements, such rules might reasonably be learned by children in the course of language acquisition. The rules are testable both internally and externally. The next question is, of course, Are they correct?

5. Testing the Rules. Testing the psychological validity of a set of rules constitutes a challenge to experimental ingenuity and requires input from a variety of sources. For example, controlled experiments dealing with ambiguity are essential in defining just what naive speakers do with ambiguous sentences. Experiments in paraphrases to see whether, and to what extent, various sentences are treated as related are logically prior the construction of a description which relates them. Studies in language acquisition can inform us to what kinds of rules are learned and in what sequence. Such empirical evidence can be called external to the extent that it deals with the psychological correctness of the rules.

Internal evidence is also important in theory construction. If it can be shown, for example, that a particular set of surface structure linearization rules formulated for main clauses is also adequate for a certain class of embedded clauses, then such rules are internally strengthened, even though such evidence does not in itself show that the rules are psychologically correct. Almost all the evidence in current linguistic theorizing is of the internal sort, with little external evidence available for anything. (See Prideaux, 1971 for a discussion of the issue of internal versus external evidence.)

At this point, then, examples of the two kinds of evidence will be examined. First, concerning internal evidence, let us consider the issue of structural ambiguity. I have chosen this example for two reasons. First, transformational theory has consistently claimed that the resolution of structural ambiguity requires the existence of deep structure. I hope to show that such a level is not needed in an information-structure approach to the problem. Second, I will try to show that the linearization rules provided for main clauses are also appropriate to embedded complement clauses, thus yielding internal support for the rules.

A sentence is considered structurally ambiguous if its two or more meanings can be attributed to distinct labelled bracketings. In transformational terms, two kinds of structural ambiguity have been proposed: "surface structure ambiguity" and "deep structure ambiguity." Surface structure ambiguity obtains in those cases where an ambiguous sentence has two

distinct surface bracketings, as in sentences like:

4 a. Old men and women sleep late.

In this sentence, old can be taken as modifying either men and women or just men. The sentence can be bracketed in the two following ways:

4 b. Old [men and women] sleep late.

c. [Old men] and women sleep late.

Such ambiguity is clearly resolvable by surface bracketings, and I will have nothing further to say about it except to comment that an information-structure approach would treat old as part of the denotational information of the conjunct men and women in 4b, whereas in 4c, old would be included in the denotational information of men alone.

On the other hand, so-called "deep structure ambiguity" is represented by sentences such as the following:

5 a. The principal asked the teachers to stop smoking.

This sentence can mean either of the following:

5 b. The principal asked the teachers to stop everyone from smoking.

c. The principal asked the teachers to cease smoking.

Transformationalists claim that to resolve such ambiguities, two distinct deep structures are needed, one corresponding to each of the two meanings. It is also claimed that such sentences have only one surface bracketing for the two meanings. I have argued elsewhere (Prideaux, 1972) that such

sentences in fact have distinct surface bracketings, one corresponding to each of the two meanings. Evidence for this claim is based on paraphrase tests which show that in one case, for instance, smoking functions as a nominal, the direct object of stop, and in the other that stop+ smoking is a compound verb.

Now let us consider a sentence like:

5 d. The principal asked the teachers to stop smoking cigars.

which is clearly unambiguous. Where, then, does the ambiguity of 5a reside? Notice that stop can mean either cause to terminate, as in 5b, or cease, as in 5c. Furthermore, smoke can be used either transitively as in 5b and 5d or intransitively, as in 5c. In light of these facts, I would treat 5a as follows: in both readings of the sentence, the principal is subject of ask and the teachers is direct object. Furthermore, the teachers is also subject of stop. However, in one reading of 5a, I would treat smoking as a nominalized verb, a NP functioning as the direct object of stop, while in the other reading, the construction stop+smoking is treated as a compound verb, much like finish dressing, begin eating, etc. Notice that this treatment of stop+smoking is also applicable to 5d in which cigars is the direct object of the compound verb. Here, smoking cannot be interpreted as the direct object since cigars preempts that role.

Distinct labelled bracketings at the surface removes the need for an abstract underlying level of syntactic structure for the distinction, although of course, the meanings associated

with the sentence must still be represented. In a transformational theory, meaning is an interpretation of syntactic structure (at a deep or surface level), whereas in an information-structure theory information is paired with distinct surface bracketings.

Thus, an information structure approach can handle structural ambiguity just as readily as a transformational analysis, but even better, no level of abstract syntactic representation is required. How does such an analysis provide internal evidence for the linearization rules? In the following way: if smoking is a NP in one reading of 5a, then as a direct object is should follow its governing verb, and it does. If the teachers is both direct object of ask and subject of stop, it should follow ask and precede stop. And it does. In other words, the same linearization rules which apply to main clause subject and direct object placement also apply to complement clauses.

In this illustration, then, it has been shown that deep structure is not needed. Further, it has been shown that since the linearization rules have a broader scope than the domain for which they were posited, internal evidence for the rules also exists.

To illustrate an instance of external evidence, I want to mention one experimental study briefly and then present some evidence from language acquisition in support of linearization rules. In his Ph.D. thesis, Paul Fletcher (1973) investigated the question of syntactic relatedness

among members of a paraphrase set including sentences like the following:

6 a. It was John who broke the clock.

b. John was the one who broke the clock.

c. The one who broke the clock was John.

d. It was the clock that John broke.

e. The clock was what John broke.

f. What John broke was the clock.

All these sentences share the same relational information, namely that John is subject and the clock is direct object of broke. Some linguists have claimed that the sentences are complete paraphrases, all having exactly the same meaning. Yet it is fairly obvious that they differ in focus, and this is what Fletcher demonstrated. For example, in response to the question:

6 g. What did John break?

sentences 6d-f are appropriate responses while 6a-c are not. An analysis which treats all the sentences as identical in meaning is incorrect, but an analysis which treats the sentences as sharing common sentential and relational information, but differing in contextual information, is closer to the empirical facts.

Finally, let us explore the acquisition of wh-questions in English. I will contrast the usual transformational analysis of the problem with one formulated in terms of the surface structure rules presented above. The data and stages of acquisition to be discussed are taken from Klima and

Bellugi (1966). They outline three stages through which children pass as they acquire negatives and questions, but I will focus attention only on the acquisition of questions. The data collected in the study were analyzed by Klima and Bellugi in a transformational framework which might be characterized as an accretion theory of transformational rules. That is, language acquisition is seen as the acquisition of transformational rules and underlying structures. I will contrast this interpretation with one of learning surface syntactic generalizations.

Let us first lay out the stages which Klima and Bellugi established for their data. The earliest stage, Stage I, involves yes-no questions signalled by intonation only, as well as simple wh-questions. Examples are:

Stage I. (p. 200)

See hole?	Where kitty?
I ride train?	Where horse go?

Stage II contains yes-no questions again only marked by intonation (and still no inversion), and wh-questions without auxiliaries. Examples are:

Stage II. (p. 202)

You want eat?	Where my mitten?
See my doggie?	What me think?
	Why you smiling?
	What the doggie have?

The basic difference between the first two stages is that in Stage II there has been some further elaboration of sentence

structure (including the use of slightly longer utterances, use of pronouns, and the development of some inflections). Stage III involves the occasional use of auxiliaries in yes-no questions but none in wh-questions or declaratives. Examples are:

Stage III. (pp.204-5)

Does lions walk? What I did yesterday?
Did I saw that in my book? What he can ride in?
Which way they should go?

Notice that in all cases of wh-questions, the wh form is always sentence initial, just as it is in the adult language. Furthermore, there are no auxiliaries in any wh-questions, although there are some in yes-no questions. And there is no inversion in wh-questions.

Under a theory of accretion of transformational rules adopted by Klima and Bellugi, the child learns some rules, then later drops them and learns more complex rules. They suggest that at Stage I, the element Q what/where is learned as a sentence-initial form, thus allowing the child to place the wh-word in sentence-initial position. At Stage II, they posit a rule of the form:

7. S → what/where + Nucleus

where the Nucleus is the basic kernel structures (NP AUX VP). At this stage, the child, according the Klima-Bellugi analysis, retains the generalization that the wh-word is sentence initial. Yet at Stage III, the analysis changes sharply. It

is claimed that the child gives up his earlier analysis of placing the wh-word first, and rather learns to associate the wh-form with an indefinite NP, generated in its "deep" syntactic position (in the subject, direct object, etc.) slot. In addition, it is claimed that the child also learns a rule of "Interrogative Preposing" which attracts a wh-word to sentence initial position. Under such an analysis, the structure underlying the sentence "What he can ride in?" is roughly:

8. Q he T can ride in wh-Indef.

Notice, however, that under such an accretion theory, one would require that if the transformation of "Interrogative Preposing" is learned, then the input (e.g., 8) must also have been learned for the rule to apply to. Under such an assumption, one would expect children to overgeneralize and make errors. In particular, one would expect children at Stage III to utter sentences with the wh-form in a deep structure, non-initial position, as in "He can ride in what?" But as Klima and Bellugi point out, such mistakes do not occur. The absence of such forms is totally unexplained in a theory of accretion of transformational rules. Furthermore, one wonders why children use auxiliaries in yes-no questions but never in wh-questions. Shouldn't the child learn the simplest generalization about auxiliaries? Such a theory would so suggest.

If we analyze the data from the point of view of

surface syntactic generalizations, no such skewing or mistakes are predicted. The linearization rule R 3, for example, states that a wh-word is the left-most NP in a sentence. It is in such a position in all its occurrences in wh-questions and relative clauses. It is perfectly reasonable that it is learned in such a position since that is where the child hears it. This theory does not rely on an underlying syntactic representation of which locates the wh-form in non-initial position. Thus, at no stage should a child be tempted to make mistakes like "He can ride in what?" (not to be confused with "He can ride in WHAT?" which I would construe as an "echo question" and not as an information-seeking question at all). Notice, furthermore, that in all the children's wh-questions, the subject NP still precedes the verb. Functionally, one might notice that the distinction between a declarative and a yes-no question is very important early on in life, and thus the means to distinguish the two is of high value. Thus, either intonation or the use of an auxiliary is soon acquired to make the distinction. But at this early time (until late Stage III), the child has no concept of a relative clause. Notice the wh-questions of Stage III do not involve auxiliaries, simply because that the child does not need at this stage to distinguish relative clauses from wh-questions. The relative clause structure does not involve the so-called "inverted" word order, but a wh-question does. The child, not recognizing or using relative clause structures, takes the simplest path--he uses the linearization rules for WH placement and for

subject (and direct object) placement, with no need for auxiliaries. The surface structure rule theory would predict that the auxiliaries would not emerge in wh questions until the child gains a substantial control of relative clauses, at which time the child would need to establish the distinction in word orders for the two different functions. The auxiliaries are not introduced simply as syntactic devices all at once under this theory. Rather, their acquisition is linked to that of functional distinctions.

I thus conclude from these data that a transformational theory of language acquisition which employs an accretion interpretation of rules is incorrect. It predicts "errors" where none appear, and it does not place adequate importance on the functional distinctions signalled by different surface constituent orderings. A surface structure generalization theory, on the other hand, is more plausible in that its generalizations are true at the surface and therefore available for extraction from the linguistic environment. Furthermore, such a theory does not predict a class of mistakes which just do not occur. Overgeneralization has been used time and again by linguists in their study of language acquisition as evidence for a particular bit of theoretical apparatus. But in this particular area, Klima and Bellugi are strangely silent about the matter. Their only comment is that the wh-forms are sentence-initial. Strange indeed for their theory! But not at all strange for the surface generalization theory. A surface-structure theory thus has empirical support

absent in a transformational theory, and consequently external evidence is provided in favor of one theory over and against the other.

6. Conclusions. I have attempted to characterize the current state of unrest in some of the linguistic camps. I must hasten to add that by no means all linguists feel such unrest. Many are content to spin webs of abstract structures with no external evidence at all. In addition I have tried to characterize an alternative theory which in principle responsible to empirical evidence and I have attempted to cite both internal and external evidence for such a theory. It is my hope that a more common-sense attitude to the problems of linguistic theories in in its birth throes, and that empirical evidence will be of central importance in testing such theories.

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